For a long time ornithologists have been hesitating whether or no to include the tinamus of South America in the same group as the ostrich-like birds; but this hesitating spirit does not commend itself to Mr. Pycraft, who boldly says that the affinities between the two imperatively forbid their separation. And it is this innovation which leads him to reject the time-honoured title, Ratitæ.

The inclusion of the tinamus in the group renders it necessary to assume (even if we had not to do so on other grounds) that the ancestors of the ostrich and its kindred were formerly endowed with the power of flight. Further, the author regards the group as a convergent one, which has had a multiple origin from the common avian stem before this began to split up into the more specialised "Carinate" types. The cassowaries and emeus are regarded as representing the most primitive branch, which culminated, perhaps, in the more advanced ostrich. From this it apparently follows, although it is not stated in so many words by the author, that the divergence of the Ratites (to call them by their old name), including the tinamus, took place while birds still retained teeth. While this may be so, it must be confessed that some palæontological evidence in its favour would be most welcome.

It may be added that, according to the genealogical tree given by Mr. Pycraft, the loss of the teeth in birds must have taken place at a still later epoch, for we find the cretaceous Ichthyornis branching off long after the divers and ducks had been differentiated. This seemingly implies that the origin of the latter is to be carried back to the Jurassic epoch, when, so far as we yet know, Archæopteryx was the sole representative of bird life. The author promises a supplementary memoir on Apteryx, where he will, perhaps, explain how we are to get out of this difficulty.

A slight discrepancy between the aforesaid "tree" and the text likewise stands in need of explanation. On p. 264 of the latter it is stated that the ostrich-like birds "are to be regarded as polyphyletic—probably triphyletic," and yet in the "tree" we find them arising from five distinct branches.

Space does not allow of allusion to the many interesting observations on the osteology and pterylosis of the group recorded by Mr. Pycraft, but these really form a storehouse of information of the utmost value to future workers. As he himself would doubtless be one of the first to allow, opinions may legitimately differ in regard to many of the conclusions arrived at by the author, but as to the value of his investigations all opinion must be in accord. R. L.

OUR BOOK SHELF.

Researches on the Past and Present History of the Earth's Atmosphere. By Dr. T. L. Phipson. Pp. xii + 194. (London: Charles Griffin and Co., Ltd., 1901.)

IN style and scope, Dr. Phipson's book reminds us of essays submitted to the Smithsonian Institution for the Hodgkins Fund Prize, and afterwards published in the Smithsonian Report. A more or less popular description is given of the atmosphere in its various relationships to man, and in its meteorological aspects; while in many places short statements are made of observations and investigations carried out by the author himself. The book should thus prove of interest to general readers as well as to meteorologists and other students of science.

In the early chapters, the thesis which Dr. Phipson seeks to establish is "that the primitive atmosphere of the earth was nitrogen, into which volcanic action poured more or less carbonic acid and vapour, and that after vegetable life appeared, free oxygen made its appearance in the air, and has increased in quantity from those primitive times to the present day." In connection with the subject of the variations in the amount of carbon dioxide in the air, it might have been well to refer to the work of Arrhenius, Chamberlin and others on the effect of variations in the proportion of the gas in air upon the mean annual temperature, and past geological conditions.

Dr. Phipson regards argon as allotropic nitrogen or a carbide of nitrogen. The hydrogen gas driven off from meteorites when heated is, he holds, produced by the decomposition of water vapour by the meteorite during the passage through the air, or the absorption of water, the oxygen of which combines with some of the constituents of the meteorite when it is heated, thus setting hydrogen free. He refers to the variation in brightness of the star Algol as "still a mystery to astronomers," though the spectroscopic work of Vogel has placed the cause of variability almost beyond doubt. Like many other writers who have not followed closely the physical geography of recent years, Dr. Phipson believes in the Gulf Stream myth, going so far as to commit himself to the statement that "The mild climate of the British Isles is very greatly due to this immense current of warm water, without which we should be no better off, in this respect, than people who live in the Arctic circle." To understand how unfounded this statement is, we refer the author to a paper in the U.S. *Monthly Weather Review* of September 1900.

In a short chapter on meteorites the remark is made, "They are, no doubt, of the same composition as the moon; and are, I believe, minute satellites of our earth, thrown off like our larger satellite was thrown off, in the earliest stages of its existence." Here again we have statements with little evidence to support them. Nothing is known of the exact composition of the moon, so the words "no doubt" in the sentence quoted are, to say the least, gratuitous.

While, therefore, we think the book contains an interesting account of the earth's atmosphere, we suggest that in several places statements are made as if they were accepted conclusions, whereas they are often opposed to the opinions of competent authorities.

Catalogue of the Mesozoic Plants in the Department of Geology, British Museum (Natural History). The Jurassic Flora. I. *The Yorkshire Coast.* By A. C. Seward, F.R.S. Pp. xii + 341; plates xxi. (London: British Museum (Natural History), 1900.)

FOSSIL plants from Gristhorpe Bay and neighbouring parts of the Yorkshire coast are so widely distributed among museum collections that Mr. Seward's descriptive catalogue of them will be welcomed by many museum curators in Britain and on the continent. But the volume is more than a catalogue; it is a history of Oolitic plant-remains of Yorkshire, exemplified by the fine series preserved in the British Museum. In addition to the data provided by this material, the descriptions are based upon specimens in many other collections which have been examined and considered. As might have been expected, the identification of type-specimens was a difficult task, and in many cases it has been found impossible to specify the type, which fact, remarks Mr. Seward, "has afforded a practical demonstration of the need of some system for the centralisation and cataloguing of all specimens which have served for the diagnosis or illustration of new species."

In an introduction a brief historical survey is given of our knowledge of the Jurassic plants of Yorkshire, and also of the Jurassic plant-bearing strata of France, Germany and other countries which resemble those of the Yorkshire coast.

In the descriptive part of the catalogue the specimens are grouped, so far as possible, in accordance with their natural affinities. Fifty-five species are described, and are distributed as follows:—Bryophyta, 1; Equisetales, 2; Filices, 20; Cycadales, 23; Coniferæ, 9. There is a resemblance between this flora and the Wealden flora, among the common characteristics being the absence of Angiosperms and abundance of Cycads and Ferns.

In conclusion, Mr. Seward remarks: "It is in the southern tropics that we must look for existing forms which afford the most striking links between the vegetation of to-day and that which has left imperfect records in the Jurassic sediments of the Yorkshire coast. The climate was presumably more tropical than that of North Europe at the present day; there is no evidence that the plants of Jurassic times grew under conditions which induced xerophytic characters, moisture being probably abundant and favourable to the luxuriant growth of Equisetums and Ferns."

Practical Electrical Testing in Physics and Electrical Engineering. By G. D. A. Parr. Pp. vi + 392. (London: Longmans and Co., 1901.) Price 8*s.* 6*d.*

MR. PARR is head of the electrical engineering department of Yorkshire College, Leeds, and the book before us represents the instructions and experiments given to students in the practical course in that department. It is not our object to criticise that course, the value of which must largely depend on the theoretical teaching accompanying it; suffice it to say that it is apparently modelled very closely on that given by Prof. Ayrton at the Central Technical College. Whether there is sufficient justification for the publication of Mr. Parr's book must depend on whether the details of the system and apparatus are sufficiently widespread; in the majority of cases the instructions given for each experiment imply the provision of special apparatus for carrying out the test, and the instructions are given, if we may use the phrase, in terms of that apparatus. A more general description of the experiments would be of wider use, though probably not so convenient for the student actually passing through Mr. Parr's course. There is a good appendix describing the chief instruments and apparatus used, and another appendix giving solutions of the various problems raised by the experiments which we do not think so valuable, except as a labour-saving device to the demonstrator or idle student. The experiments are, on the whole, well devised to bring out clearly the fundamental laws of electricity and magnetism. We are sorry, however, to see included an experiment to "prove" Ohm's law, in which P.D. is measured by a high-resistance galvanometer; a galvanometer can only be employed to measure P.D. if Ohm's law be true, so that it cannot logically be used for this purpose in an experiment to prove the law. The method given second by Mr. Parr, in which an electrometer is used to measure P.D., is the only satisfactory one for proving Ohm's law. Technical teachers who are seeking to develop a practical course will find this volume a valuable guide.

LETTERS TO THE EDITOR.

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Audibility of the Sound of Firing on February 1.

IT is an interesting question how far the accounts of various observers as to the sound of the minute guns on February 1 having been distinctly heard at many distant stations and not heard at others comparatively near by attentive listeners, and as to the character and duration of the sound, can be explained by known laws of the propagation of sound in the atmosphere. To this question I should like here to offer such answer as I have been able to arrive at after careful consideration and some rough calculation.

The firing line extended from the *Majestic* at the eastern end to the *Alexandra* at the western, in a direction some 6° N. of W. for about 8 miles. The eastern half was a double line of 16 pairs of ships, the distance between the two lines being about ¼ mile, nearly the same as that between the successive ships in each line (2½ cables or ¼ sea mile); while the western half was a single line of 14 ships.

Some stress has been laid by observers near the firing line on the want of simultaneity in the discharges from the different ships. There is doubtless need of more accurate information on this point, but I cannot help thinking Mr. Hinks's estimate that, as "the firing ran down the double line, the interval between the successive pairs of flashes was about half a second," is excessive. This would add, to an observer at Southsea, 8 seconds over and above the 19 to 20 seconds by which the sound of the westernmost pair of the double line was necessarily behind

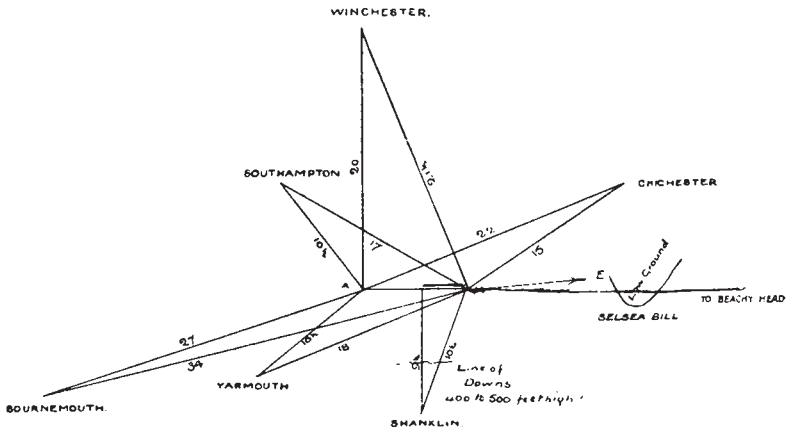

FIG. 1

that of the first pair, whereas he says that the whole sound "lasted only about 20 seconds." I do not think, therefore, in the absence of more exact information, account need be taken in a general explanation of the supposed want of simultaneity, which I estimated on such information as I could get as not amounting to a difference of more than 2 or 3 seconds.

Mr. Hinks's observation is in another respect important, as it would seem to show that the second half of the firing line was unheard at the distance of 4 to 5 miles from its eastern end.

The known causes which would or might affect the audibility of the sound impulses arriving at any station are, I think, these: the position of the station relatively to the firing line, the direction of the wind and the variation in its speed with the height above the earth's surface, the variation in temperature of the different strata of the atmosphere, and for greater distances the curvature of the earth's surface.

Of these, for distances not exceeding 30 to 35 miles from the firing line, I believe the dominant factor to be the first, that is, relative position of the station.

The accompanying diagram (Fig. 1), drawn roughly to scale, shows this, I think, conclusively.

At Shanklin the reports were heard, but they were not very